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GEODESY.—*Our Northern Boundaries.*[1] O. H. TITTMANN. Coast and Geodetic Survey.

In accordance with the custom of this Academy, which prescribes a theme in which the retiring President is especially interested, I have chosen "Our Northern Boundaries."

It happens that your speaker has been engaged for the last ten years as Commissioner on the part of the United States, in co-operation with Dr. W. F. King, the British Commissioner, in the formidable task of delimiting the Alaska boundary and re-marking and establishing in precise manner the boundary from the Pacific Ocean to Lake Superior and from the St. Lawrence River to Grand Manan Channel.

It would lead too far afield in an address of this kind to enter into a detailed account of the complications and controversies which had to be settled by various commissions before the treaties negotiated by Secretaries of State John Hay and Elihu Root afforded the means and prescribed the method for finally fixing our northern boundaries. My purpose is to confine myself to a few illustrative references of their origin and in this connection I recall an incident which may serve to suggest the cause of many difficulties in boundary questions. While serving on the staff of General Foster, the American agent before the Alaskan Boundary Tribunal which convened in London in 1903, a banquet was given at which I was placed at a narrow table immediately oppo-

[1] Address of the retiring President of the Washington Academy of Sciences. Delivered before the Academy on Thursday evening, January 14, 1914, at the Cosmos Club.

site Lord Alverstone, Chief Justice of England, who presided over the Tribunal. In introduicng me to two gentlemen, between whom I sat, he explained that they had been associated with him in the consideration of various grave international boundary questions and that they had all come to the conclusion that maps were very unreliable evidence.

Having in mind that our contention before the Tribunal involved the use of maps, the obvious answer was that some maps were doubtless unreliable.

This difficulty of the unreliability of maps was encountered immediately after the signing of the Treaty of Peace of 1783, in regard to our northeastern boundary. For, that treaty carried the boundary down the middle of the St. Croix River to its mouth. This, in the light of our modern geography seems quite explicit, but after the lapse of eleven years it became necessary to appoint a commission to decide upon the identity of the St. Croix River. This identification was successfully accomplished under the Treaty of 1794. But how about the line from the mouth of the St. Croix thru Passamaquoddy Bay?

Its course was defined in the Treaty of Peace of 1783 to be such as to assign to the United States "all islands within 20 leagues of any part of the shores of the United States and lying between lines to be drawn due east from the points where the aforesaid boundaries between Nova Scotia on the one part and east Florida on the other shall respectively touch the Bay of Fundy and the Atlantic Ocean, excepting such islands as now are, or heretofore have been, within the limits of the said province of Nova Scotia."

This general provision was difficult of interpretation because both governments claimed some of the islands. Several treaties which failed of ratification were proposed. One of them contained an extraordinary provision which would have placed Campo Bello Island within the limits of the United States boundaries, but provided that it should remain under British jurisdiction.

Thus, there was no settlement of the disagreement of the two governments until the Treaty of Ghent in 1814, the beginning of the hundred years of peace which we are to celebrate in December of this year. Under that treaty Commissioners were appointed

for the settlement of the dispute and altho they decided on the ownership of the principal islands, they were not required to lay down the line. Their decision rendered in 1817, may therefore be likened to a first approximation of the solution of the problem. Three-quarters of a century later, under the convention of 1892, Commissioners were again appointed and they marked a part of the line and disagreed as to the rest, principally because the nationality of a little islet, less than half an acre in extent, and lying in the course of the boundary, had not been specifically established by the Commissioners under the treaty of 1814. This was a second approximation. One hundred and eighteen years after the Treaty of Peace of 1783, it became possible to fix this part of the line by courses and distances and by reference to monuments on the ground and it may surprise you to know that the line from the mouth of the St. Croix to its headwaters has never been laid down on a chart, or marked on the ground, except in so far as the work of the present commission has progressed.

These complications in the settlement of the boundary thru Passamaquoddy Bay, briefly outlined in the foregoing, are typical of the process of the delimitation of the whole line.

In 1803 Jefferson, in his annual message to Congress, said that a further knowledge of the ground in the northeastern and northwestern angles of the United States had evidenced that the boundaries established by the Treaty of Paris between the British Territories and ours in those parts, were too imperfectly described to be susceptible of execution.

At one time a part of the northeastern boundary matter was submitted to the King of The Netherlands for arbitration, but his decision rendered in 1831 was rejected by both governments. A remarkable example of difficulties caused by unreliable maps or surveys is afforded by that portion of the line which forms the boundary between the State of New York and the Province of Quebec. As early as 1606, James I proclaimed the 45° parallel as the boundary between the Provinces of New York and Quebec, and between 1771 and 1774, two surveyors, Valentine and Collins, marked the boundary supposedly along the 45th parallel. When the Commissioners under the Treaty of Ghent attempted to trace

the line along the 45th parallel, called for by the treaty, it was discovered that the 45th parallel was about three-quarters of a mile south of the Valentine-Collins line and that the United States had built a fort costing about a million dollars at Rouses Point, north of the 45th parallel and therefore under the treaty definition in British territory. This discovery was made in 1818 and caused a dispute which was settled by the Webster-Ashburton treaty of 1842 under which the Valentine-Collins line was adopted by a compromise in regard to other disputed parts of the boundary.

Here I may digress from my subject for a moment to interject the remark that the Commissioners under the Treaty of Ghent missed their opportunity of demonstrating the great practical value of one of the most useful and precise astronomical instruments, the Zenith telescope. For they were equipped with one, but failed to use it, because its verticality was dependent on a plummet. A similar instrument, which however was equipped with a level, was in the possession of the Coast Survey and this was transferred, "ceded" as Superintendent Hassler said, to Lieut. Talcott for the purpose of another boundary survey in 1833 and to him fell the distinction of developing and applying the method of determining latitudes with the Zenith telescope.

We may resume the story of treaties based on incorrect maps, at the northwestern angle of the Lake of the Woods. The Treaty of 1783 provided that the boundary should run from the northwesternmost point of the Lake of the Woods, on a due west course to the Mississippi. A glance at the map will show the impossibility of such a line and the fact was recognized in the Treaty of 1794 by the admission that the course and location of the Mississippi was uncertain and, in the Treaty of 1818, it was provided that if the northwesternmost point of the Lake of the Woods was not on the 49th parallel, a line should be drawn north or south to the 49th and the boundary should run from their intersection due west to the Stony Mountains. By the time of the Webster-Ashburton Treaty in 1842 it was known that the northwesternmost point of the Lake of the Woods was north of the 49th parallel and the description of the boundary was accordingly modified.

In this cursory account we have now reached the summit of the Rocky Mountains. The controversies connected with the Oregon question were formally settled by the Treaty of 1846 which defined the boundary from the summit to the Pacific Ocean.

That portion of the boundary which was described as running to the middle of the channel which separates the continent from Vancouver's Island and thence southerly thru the middle of said channel and of Fuca's Straits to the Pacific Ocean gave rise to a serious and prolonged controversy as to which channel was meant, a controversy in which insufficient charts played but a minor part, but which would have been avoided if the treaty makers had laid down the line concurrently with the making of the Treaty. This dispute was submitted to the German Emperor who decided in favor of the United States by the award of October, 1872.

A review of the condition of the demarcation of the northern boundary when Secretaries Hay and Root undertook to perfect it, discloses that from Grand Manan Channel to the headwaters of the St. Croix, a distance of 110 miles, the boundary had not been marked on any series of maps nor had reference monuments been placed on the ground.

From the headwaters of the St. Croix to the River St. John it was monumented. Thru the St. John and to the headwaters of the St. Francis it was marked on treaty maps. Thence to the St. Lawrence it was monumented and laid down on maps.

Thru the Great Lakes and thru the water communication, to the northwestern angle of the Lake of the Woods, a distance of 1500 miles it was unmarked, but was laid down on inaccurate maps and therefore impossible of definition on the ground by either government acting for itself.

It was marked at intervals from the Lake of the Woods to the Gulf of Georgia, but in the region between the summit of the Rocky Mountains and the Gulf of Georgia, a distance of 410 miles, about 220 miles had never been surveyed, traced or monumented. From Point Roberts on the Gulf of Georgia to the Pacific Ocean it was laid down on a chart, but had never been referred to objects on shore or defined with such accuracy that it could be reproduced without question. This condition was giving rise to international questions on different parts of the boundary.

In connection with the re-marking of the boundary from the summit of the Rocky Mountains westward, it developed that no trace could be found of the report of the American Commissioners on the delimitation of the boundary between 1856 and 1869. By a singular coincidence and fatality no trace could be found by the British Government of the records of its Commissioner, altho copies of the maps were on file in both countries. The circumstances connected with the discovery of the British records have perhaps never been published and may be here recorded. Pending the consideration of the marking and re-marking of the line the Dominion Government despatched Dr. Otto Klotz, a Canadian astronomer, to London to make another attempt at their recovery. He searched the archives of the Foreign Office and War Department, but in vain. While on a visit to the observatory at Greenwich, he happened to note a box on one of the upper shelves with the letters "B.N.A." inscribed on it. He interpreted them to mean British North America and their possible significance flashed thru his mind. He asked what the box contained. No one knew. A ladder was sent for and the box opened and the records were found.

The relevant and important parts were printed for the use of the Canadian Government. A copy was furnished to this government. This happy find by Dr. Klotz doubtless prevented complications which might otherwise have arisen as to the details of the boundary and obviated the necessity of a new treaty or of special provisions in the general treaty already referred to in the beginning of my address as having been negotiated by Secretary Root. Under this Treaty of 1908, the task of marking and re-marking the boundary was assigned to two Commissioners. The definition of the line through the Great Lakes was assigned to the existing International Waterways Commission and extends through a distance of about 1200 miles.

The survey and re-marking of the remainder of the distance, about 2500 miles, that is from the Pacific Ocean to Lake Superior and from the St. Lawrence to the Grand Manan Channel, was entrusted to the Superintendent of the Coast and Geodetic Survey, representing the American Government and Dr. W. F. King,

Chief Astronomer of the Dominion of Canada, representing the British Government.

To avoid wearying you with details, I have placed against the wall a map which will convey to your eyes the progress made. It indicates that a careful trigonometric and topographic survey has been made of the greater part of the line and that comparatively little of the field work remains to be done on the boundary under consideration. The labors of the International Waterways Commission, it is understood, are also drawing to a conclusion and thus we shall soon have a thoroly marked boundary and be in possession of accurate maps and a precise definition of the whole line as the result of processes based on sound engineering principles.

## THE ALASKA BOUNDARY

Included in my subject is the delimitation of the Alaskan frontier, but this chapter I shall treat very briefly, as the circumstances connected with it are fresh in your memories.

The Alaskan boundary naturally divides itself into two sections. The first is the 141st meridian of west longitude and runs from the Arctic Ocean to a point about 10 marine leagues from the coast, a distance of 645 miles. About this stretch there never was any dispute, but it was deemed best to prescribe by a treaty, signed at Washington in 1906, the manner of ascertaining the location of the 141st meridian and of tracing the line.

Our knowledge of geodesy makes it evident that if in a stretch of 600 miles various points should be determined astronomically on the 141st meridian, the resulting line would be a zigzag, following the intersection of the verticals with the ground, that is, the meridian would not lie within the plane of one geometric great circle.

The Commissioners were therefore directed to determine the 141st meridian by telegraphic longitude at a suitable point thru which they were to trace a north and south line. This point having been established at the crossing of the meridian and the Yukon River, a zealous, able and energetic corps of engineers cut a line thru the timber from Mount Natazhat on the south, to the Arctic Ocean, monumented it and made a careful trigonometric

and topographic survey, extending about 2 miles on each side of the boundary. Aside from its immediate purpose this monumented line and attendant triangulation will serve as a most valuable base for the orientation of future topographic and economic surveys both sides of the line. In the main this distance was a transit line, but owing to the difficulty of traversing the ice fields and rugged mountains, between Mount Natazhat and Mount St. Elias, a trigonometric survey was carried for a distance of about 150 miles, around by way of the Scolai Pass to the meridian south of Mount Natazhat to a point on the slopes of the Elias Alps.

It gives me pleasure to announce to the Academy that the Commissioners believe that the field work along the 141st meridian is completed. The reservation implied by the word "believe" means that the final plotting of the work between Mount Natazhat and Mount St. Elias has not been completed and that this plotting may develop a topographic lacuna in the ice fields of that region.

The other section of the line extending from Mount St. Elias to the Portland Canal and thence thru Dixon Entrance to Cape Muzon shared the fate of our other boundaries in being a subject of controversy which was happily settled by the Tribunal of London which rendered its decision in 1903. It is interesting to recall, however, that at the time of the Alaska Purchase, Senator Sumner, in advocating the purchase, began his scholarly speech with the following words: "In endeavoring to estimate its character, I am glad to begin with what is clear and beyond question. I refer to the boundaries fixed by the treaty."

The correctness of this statement was not borne out by subsequent events. Thirty-six years elapsed before the ensuing disputes were settled by arbitration. Under the terms of the arbitral award, Messrs. King and Tittmann were appointed to effect the delimitation on the ground. Excepting some trigonometric determinations and a final monument or two this has been accomplished. The boundary peaks have been determined trigonometrically, a photo-topographic survey has been made and monuments have been placed at important and accessible points and now the preparation of the final maps for the whole boun-

dary from Cape Muzon to the Arctic Ocean, a distance of about 1600 miles, measured along the frontier is in progress.

All honor to the distinguished statesmen of both countries who successfully strove to find a peaceful settlement of the many controversies which have been barely touched upon by me. Their justification lies in the homely adage, that, "Good fences make good neighbors."

PHYSICS.—*Micrometer microscopes.* ARTHUR W. GRAY. Bureau of Standards. To appear in the Bulletin of the Bureau of Standards.

The purpose of this communication is to outline a simple and rapid method of applying the proper corrections to the readings of micrometer microscopes when making length measurements of precision, and also an accurate graphical procedure for computing convenient correction tables. The numerous sources of error that need attention will not be discussed in this brief paper.

TABLE 1

| | A | B | C | D | E |
|---|---|---|---|---|---|
| 1 | +151 | −38 | −1 | −8 | −15 |
| 2 | +102 | +27 | 0 | +1 | − 9 |
| 3 | +101 | −21 | +1 | +8 | +12 |
| 4 | − 40 | −49 | +1 | +7 | + 9 |

In order to emphasize the advisability of examining a microscope intended for accurate measurements Table 1 is introduced, which shows the magnitudes of the periodic errors found in some microscopes. On the assumption that the micrometers read correctly at the beginning and at the end of a turn, the corrections in ten-thousandths of a turn that must be added to the reading at the end of each fifth turn indicated in the first column are recorded in the other columns. Under A are the corrections for one turn of a microscope that had been repaired as well as possible after its micrometer thrust bearings had been discovered scored by a grain of dirt. Under B are those found for a turn of a microscope used in making important length measurements. The instrument had

never before been investigated. A curious feature of this microscope was the regular repetition of this series of corrections every other turn, with a somewhat different but equally regular series for the alternate turns. Column C represents a screw of unusual excellence, made by a firm noted for high-grade astronomical instruments. D represents the average and E the worst of a group of ten others made by this same firm. The irregularities which these ten exhibit are of the same order of magnitude as those of the best microscopes belonging to the International Bureau of Weights and Measures.

While the need of applying corrections to some of these microscopes is perfectly evident, it may be well to point out that screw irregularities are of importance even in such micrometers as those of the International Bureau of Weights and Measures. An examination of the correction tables appended to one of Benoit's papers[1] shows that an error of about 1.5 $\mu$ can be made in comparing two lengths if the only corrections applied are those for deviations of the mean screw values from the nominal value of 100 $\mu$ per revolution; and further that most of the error will be due to neglecting the *periodic* corrections.

In comparing two lengths with a transverse comparator it is a common practice to multiply the changes in each micrometer reading by the mean scale-value and subtract. But even less labor is required to take into account *all* necessary corrections, after the microscopes have been calibrated once for all. The writer's method of doing this is essentially a simplification of the procedure followed by the International Bureau of Weights and Measures in measuring the national prototype meters.[2] The principal gain is effected by reducing to two tables (both of which can be mounted on a single card) all the information contained in the six tables used by the International Bureau. The table for the left microscope gives at one inspection the total correction $\Delta L$ that must be added to any reading $L'$ of the left microscope to give the corrected reading $\Delta L$; that for the right microscope gives $-\Delta R$, the

[1] J. R. Benoit: Mesures de dilatation et comparaisons des règles métriques, Trav. et Mém., **2**, C. 131, 1883. The tables are on pp. c, xi, and xii.

[2] Described in the above mentioned paper by Benoit.

negative of the total correction to the reading $R'$. Reversing the sign of $\Delta R$ is merely a matter of convenience. It is usually sufficient to enter the corrections for every tenth of a turn and to obtain those for the hundredths by interpolation. A convenient arrangement is one similar to that followed in most mathematical tables: the whole turns are indicated at the side and the tenths at the top.

The displacement corresponding to one division (= 0.01 turn) of the microscope may differ so little from 1 $\mu$ that only small corrections will be required to reduce the readings to microns. It may however, well happen that reduction to the desired length unit might require corrections so large as to be inconvenient. It then becomes advisable to select an auxiliary unit corresponding to the average screw-value, and to express all corrections in terms of this unit, which should be *the same for both microscopes*.[3] It is further advantageous to minimize the magnitude of the corrections by adjusting both microscopes to give almost the same magnification—if possible one that permits a simple reduction factor. Then all computations can be made in terms of the auxiliary unit, multiplication by the reduction factor being deferred until the end.

Another gain is effected by recording observations in such a form that the corrections can be added directly without any copying of figures. For example, the complete record and computation for a single comparison of two bars A and B would appear about as follows:

| | A | B |
|---|---|---|
| L' | 3027.9 | 2431.9 |
| R' | 2938.1 | 3505.7 |
| L'−R' | 89.8 | −1073.8 |
| $\Delta$L − $\Delta$R | 3.0 | −1.4 |
| L − R | 92.8 | −1075.2 |

[3] All these considerations apply with increased force to a larger group of microscopes, for example; a group of four used to read a divided circle, or several used to step off a considerable length by means of a bar reaching from one microscope to the next.

$\therefore L_A - L_B = 1168.0$ divisions. Since the magnification by the microscopes was such that 1 division $= \frac{3}{4}\ \mu$

$$L_A - L_B = \tfrac{3}{4} \text{ of } 1168.0 = 876.0\ \mu.$$

The accuracy that can be obtained with microscopes having errors even as large as those listed under A in Table 1 is shown by measuring three times with each of a pair the same interval of $999.05\ \mu = 1332\ (\frac{3}{4}\ \mu)$. The results are recorded in Table 2 below. In each series the center of the field was located consecutively near the right edge, the center, and the left edge of the image, so as to obtain wide variations in the corrections, which were taken from a correction table of the type described. Part of the deviations from the averages are to be ascribed to inaccurate focusing and another part (perhaps )to linear interpolation, which is not altogether justifiable with such large and irregular periodic errors. Nevertheless, the agreement is even better than would have been predicted.

The calibration of a microscope by one of the processes usually described involves such laborious computations and least-square adjustments that it is no wonder to find it attempted only as a last resort when the demands of high accuracy compel. While, of course, the complete calibration of any divided scale requires considerable time, it is, however, possible to determine microscope corrections more accurately and with less labor than would appear from previous accounts. The whole procedure can be made one of direct measurement and simple graphical addition.

In determining the screw errors it is customary to use the ocular of the microscope under investigation for viewing the displacement of the spider-lines. An enormous gain, however, is secured by removing the ocular and measuring the displacements by means of a second micrometer microscope.[4]

[4] The use of an auxiliary microscope for this purpose was first brought to my attention by Mr. E. D. Tillyer, of the Bureau of Standards, to whom I am indebted for many valuable suggestions and criticisms. He informed me that it was the regular practice at the United States Naval Observatory. (See J. C. Hammond: Introduction to Publications of U. S. Nav. Obs. **6**, A XIII. 1911.) Sir David Gill (Roy. Astrom. Soc. Monthly Notices **45**, 65. 1884) also employed a compound microscope in a simple apparatus he designed for rapidly and accurately investigating screw-errors; but his method is in several respects inferior to that of the Naval Observatory.

With this second microscope the following distances are determined: (1) That traveled by the spider-lines as the screw ($S$) under investigation is advanced five whole turns. (A different number of turns may sometimes be more convenient.) (2) That traveled during each single turn. (3) That traveled during each fifth (or tenth) of a turn. It is not necessary that the screw of the auxiliary microscope ($A$) be very accurate, because all the pointings will be confined to the same portions of this screw, each only a fraction of a turn long. Besides, any irregularities here will be rendered negligible by choosing an objective that will magnify the interval to be measured sufficiently to make its image nearly fill the usable portion of the field of view. The measurement of consecutive five-turn intervals for the entire length of the screw is not necessary, but it is advisable in order to reduce the acumulation of errors by addition in determining the progressive corrections; and the extra time consumed is a small item. Again, it is hardly worth while to carry out measurements (2) and (3) with every turn. It will usually be found sufficient to measure the first and the last turn of each five-turn interval. Since adjacent turns are not likely to differ much, this grouping in pairs will check blunders.

After all the intervals have been measured, a length close to the average for one turn of $S$ is selected as a basis of comparison. A multiple of ten divisions of $A$ is convenient. By simply adding the deviations of the measured five-turn intervals from five times this basis, we compute the progressive corrections at the beginning of each five turns, choosing the point of zero correction at the middle of the screw. These corrections will be expressed in divisions of $A$, and the same unit is retained in all stages of the computation until the end, when the correction curves are read off by a scale which automatically translates into the numbers to be entered in the final table.

We now plot a series of curves the abscissae of which are the whole turns of $S$, that is, the comb readings of the micrometer to which it belongs. The ordinates of the first curve ($C_0$) are the progressive corrections mentioned in the preceding paragraph; those of the second ($C_{100}$) are the lengths of the one-turn inter-

vals; those of the next five ($_0p_{20}$, $_{20}p_{40}$, $_{40}p_{60}$, $_{60}p_{80}$, $_{80}p_{100}$) are the lengths of the one-fifth turn intervals in proper order. The next step is to adjust these curves so that they are mutually consistent, that is to say, the progressive corrections found by adding five consecutive ordinates of $C_{100}$ should agree with the corrections represented by $C_0$, and any ordinate of $C_{100}$ should equal the sum of the corresponding ordinates of the curves $_0p_{20}$, . . . . $_{80}p_{100}$. If the curves are plotted to a suitable scale the additions are quickly made by marking off the distances on strips of paper. These adjusted curves afford a fairly safe means of interpolation to obtain the corrections for intervals not actually measured. The curve plotted from the sums of the components of each interval is likely to lie above or below that plotted directly from the entire intervals. This divergence can proceed from progressive error or gradual change of scale value in the screw of the auxiliary microscope, or from thermal expansion of S; but it is of no consequence. The adjustment is first directed towards rendering the two curves parallel, then any distance separating them is divided among the curves representing the components of the interval by merely shifting the base lines from which the deviations would be measured.[5]

After the adjustments have been completed, the curves for the one-fifth turn intervals are added graphically to form a new set ($C_{20}$, $C_{40}$, $C_{60}$, $C_{80}$) yielding the corrections at the beginning of each fifth. If interpolation is necessary, it is readily accomplished graphically with sufficient accuracy. The interpolating curve for any selected turn will exhibit the corrections at every drum reading within this turn. Five points besides the initial zero are obtained from the correction curves just mentioned. If interpolating curves are plotted for several turns evenly distributed along the course of the screw, sufficient data can be obtained for plotting curves ($C_{10}$, $C_{30}$, $C_{50}$, $C_{70}$, $C_{90}$) to give the correction at every tenth of a turn, or oftener, as functions of the comb readings.

[5] Altho adjustments of this kind made by different computers would probably differ slightly, a least-square solution would be a sheer waste of effort, since the differences in almost any case would affect the final corrections by considerably less than the accidental error of a pointing in using the micrometer of which S forms a part.

All that now remains is to determine the magnification and to translate these curves into the figures that forms the final table.

It is, of course, desirable that the numbers in the correction table, and also the tabular differences, be small. Tabular differences arising from periodic irregularities cannot be reduced except by improving the screw. Progressive errors and deviations from the nominal screw-value, however, even if small, lead to the accumulation of large corrections when many turns must be used. But if the magnification is adjustable, these corrections can often be materially reduced. The first of the series of curves plotted, namely, that representing the progressive errors, was based on deviations of the measured lengths of individual turns from the length represented by the basis selected for comparison. If a known length, such as a standard millimeter, be measured with the microscope to which the screw $S$ belongs, and the readings corrected by using the curves described above, the distance measured that would correspond to the basis of comparison is likely to differ somewhat from the distance to be represented by 100 divisions (one turn) in the correction table. A little reflection will make it apparent that corrections for this divergence can be added to the progressive corrections by the simple device of drawing a line at the proper angle thru the point selected for zero correction. The distance parallel to the axis of ordinates between this line and the progressive curve ($C_0$) measures the total correction for any comb reading. It is, therefore, evident that these corrections can be reduced to a minimum for any given part of the screw by adjusting the magnification of the microscope so as to give this line the most favorable slope. A moment's inspection of the progressive curve will decide what this should be. After the magnification has been adjusted with sufficient closeness, a standard length interval should be carefully measured, and the correcting line accurately located. Several measurements using different parts of the screw will afford valuable checks on the accuracy of the correction curves. Table 2 represents measurements of this kind.

We are now ready for the final step of reading off the corrections from the curves and entering them in the table. A translating scale is used which reads the plots in terms of the unit chosen for

expressing the corrections in the table. A piece of co-ordinate paper cut diagonally at the proper angle forms a convenient scale. The corrections at the beginning of each turn are measured by the distances between the curve of progressive corrections and the inclined line correcting for magnification. The corrections at the

TABLE 2

Measurement of 999.05$\mu$ = 1332 ($\frac{1}{4}\mu$)

| UNCORRECTED | CORRECTIONS | CORRECTED | DEVIATIONS |
|---|---|---|---|
| 1341.5 | −8.7 | = 1332.8 | +0.8 |
| 1333.1 | −1.4 | = 1331.7 | −0.3 |
| 1337.1 | −5.5 | = 1331.6 | −0.4 |
| Averages with left microscope | | 1332.0 | ±0.5 |
| 1319.5 | +13.1 | = 1332.6 | +0.5 |
| 1316.0 | +15.5 | = 1331.5 | −0.6 |
| 1318.7 | +13.6 | = 1332.3 | +0.2 |
| Averages with right microscope | | 1332.1 | ±0.4 |

tenths of a turn are read from the corresponding curves, the base line of each being shifted if necessary to correct for magnification within a turn. (Progressive errors within a turn as well as periodic errors were cared for in deriving the curves.) These internal corrections must, of course, be added to the correction at the beginning of the turn before entering in the table.

ELECTROCHEMISTRY.—*The silver voltameter. Part IV.* E. B. ROSA, G. W. VINAL, and A. S. McDANIEL. To appear in full in the Bulletin of the Bureau of Standards.

In the three papers preceding this, the course of the work has been traced from its beginning in 1908 until the International Technical Committee met in Washington in April, 1910, to carry out a joint investigation of the voltameter and to determine the voltage of the Weston Normal Cell in terms of the international ohm and the international ampere, the latter being derived from the silver voltameter. The object of the present work is to make a further comparison of the porous cup and Smith forms of voltameter, since these have been found the most reliable forms dur-

ing the committee's work and to make such other experiments as might afford data for the voltameter specifications, not yet adopted.

The International Committee fixed the voltage of the cell to five significant figures (1.0183 volts at 20°C.), but it is desirable to reach an accuracy sufficient to justify recording six figures, since the voltameter is a primary standard. Results are expressed as the voltage of the Weston Normal Cell at 20° computed from the defined electro-chemical equivalent of silver, 1.11800 mg. per coulomb.

The procedure was according to the best methods learned from previous work. In all cases the electrolyte was carefully tested for its purity and the acidity determined. The voltameters used were chiefly the porous pot and Smith's form, but on several occasions we used the siphon form, the modified ring-shaped siphon, and the Poggendorff form.

Since we had learned from previous work that the effect of acid in pure electrolyte is to lower the deposit, we sought to find if possible a quantitative relation between the amount of acid, expressed in equivalents of $HNO_3$ per million, ($X$) and the decrease in deposit, also expressed in parts per million, ($Y$). As a result of a large number of comparisons of neutral voltameters with others made acid by varying amounts we have found the following relation

$$Y = -4.5X + 0.02\,X^2$$

which we think is justified by the figures given in the complete paper.[1]

For acidities below 10 parts per million the square term is of course unnecessary. The above equation was used in computing a correction to each deposit, but this correction was very small in all cases for the results given below. The result of 156 deposits in the porous cup form is:

$$1.01826_7 \text{ volts}$$

The average deviation of a single observation from the mean is

[1] Von Steinwehr's recently expressed opinion to the contrary (Instrumentenkunde, November 1913) was based on earlier work which was not so conclusive as our recent work.

$0.00003_0$ and the computed probable error of a single observation is $0.00002_5$. The probable error of the mean result is $0.00000_2$.

The result of 55 deposits in the Smith form is:

$$1.01827_4 \text{ volts}$$

The average deviation of a single observation from the mean is $0.00004_0$ and the computed probable error of a single observation is $0.00003_4$. The probable error of the mean result is $0.00000_5$.

The mean of the porous pot form and the Smith form is therefore

$$1.01827 \text{ volts}$$

which we give as the final result. The electrolyte was so pure that no volume effect was observed in either form of voltameter.

With other forms of voltameter we have obtained the following results: Siphon form (relative experiments, 5 observations) 1.01832; Modified siphon (7 observations) 1.01835: Poggendorff form (9 observations) 1.01830. We thus have results with five forms of voltameter all confirming the decision of the International Committee, but the difficulties arising from the anode slime in the last two make them more uncertain and difficult to use. The siphon voltameter requires so much electrolyte that the presence of the slightest trace of impurity increases the "volume effect" noted previously in several other forms.

We made a number of miscellaneous observations on the effect of various impurities, but will mention here only the excessive deposits obtained when considerable $CO_2$ is present in the atmosphere above the voltameters. Two possible theories are given in the complete paper to account for the slight difference occasionally appearing in the deposit between the Smith form and the porous pot form.

A further study of the effect of acid on solutions *known to be impure* has shown the effect to be quite variable depending on the nature of the impurity. This fact we believe accounts for much of the conflicting opinion found in the literature on this subject. We have endeavored to find just what the action of acid is by which it reduces the deposit from pure electrolyte, but our results are chiefly negative. It seemed possible that in the case of an

electrolyte appreciably acid the decrease might be due to a depositing out of the hydrogen ions before the silver begins to deposit, since there is probably at the instant the current begins to flow a greater difference of potential at the cathode than obtains later, as the discontinuous character of the deposit indicates that the potential difference must break thru a sort of surface film and establish outlets for the current to the platinum cathode. If this were possible, by making deposits of about 1 mg. each from solutions containing acid and without acid, the difference ought to be apparent. This we have done for varying lengths of deposit, measuring the silver by titration with ammonium sulphocyanate. The results showed always an agreement of both deposits between themselves and with the computed value for the silver within the experimental error. But when we electrolyzed oxygen into the platinum cathode for four hours previous to making a deposit a loss of about 0.2 mg. was observed, this was not again found on subsequently using the bowl unless it had been oxygenated as before.

We have made a careful study of the effect of various septa other than filter paper on the voltameter. First raw silk was investigated. Extracting raw silk in neutral double distilled water we find the extract to be distinctly basic toward both methyl and iodeosine and the concentrated extracts will restore the color to magenta solution previously decolorized by sulphurous acid, thus indicating the presence of an aldehyde. The colloidal character of the solutions was ascertained by the ultra-microscope. After prolonged washing or use in the voltameter the so-called "silk glue" which probably gives rise to the above effects is eliminated and a hydrolytic process into amino acids appears causing a reverse action in the voltameters. We have never claimed that these two actions take place at the same time, as implied recently by von Steinwehr. We think that silk like filter paper should be excluded from use in the silver voltameter.

Our experimental evidence indicates that porous pots if properly prepared are without influence on silver nitrate. A new pot or one that has been baked may contain free alkali which must be washed out with nitric acid and water until perfectly neutral

and then several portions of silver nitrate filtered thru the pores until equilibrium between the pot and solution is obtained since at the beginning the pot acts as a catalyzer, to reduce a very small amount of solution. After being properly prepared there is no evidence of stenolysis or other abnormality in the ordinary use.

The question of the purity of the deposited silver is an important one, but the conflicting evidence in the literature does not enable one to decide even whether inclusions are present or not. The uniformity of the results that we have obtained in three series of experiments suggests that the inclusions must be very small or surprisingly constant in amount. It seems to us that inclusions if present would be between crystals rather than in a crystal perfectly formed according to the cubic system and hence greater inclusions are to be expected in deposits which are less crystaline because of the action of colloids.

We have endeavored to detect the presence of $AgNO_3$ in the deposits by amalgamating them with mercury then washing the amalgam and testing for nitrates by phenol sulphonic acid. Our blank experiments, adding $AgNO_3$ to the amalgam after precipitating any mercury and silver salts present worked satisfactorily, but we have been unable to detect any appreciable $AgNO_3$ in the best deposits altho a trace was found in striated deposits. If considerable filter paper was used some organic matter was visible after amalgamating the deposit. We tried to determine the water content of a striated deposit having an excess weight over a normal deposit of 0.56 mg. by alloying the deposit to the platinum crucible in which it was made over an alcohol blast, and found a loss of 0.053 mg. which previous experiment showed was not due to the change in the platinum crucible alone. We were not able at this time to make further experiments and therefore do not consider the result conclusive.

The reactions in the voltameter have not generally been considered as reversible, that is, an equivalence of the loss in weight at the anode and gain at the cathode, but since our previous work has shown that the supposed complex reactions at the anode do not probably exist we examined the possibility of reversibility. For an anode we used a heavy deposit contained in a large platinum

bowl and connected the electrolyte in this thru a siphon to a second bowl serving as cathode. First we measured the progressive change in acidity of the anode solution as electrolysis took place and found that practically no change of acidity occurs until the silver on the anode was reduced to about 0.5 gram after which it rose very abruptly. Next we measured the potential difference between the anode and an intermediate electrode and found this also constant until about 2.5 gram of silver remained. We then examined the question quantitatively as follows:

| WEIGHT OF SILVER ON ANODE BOWL | CURRENT | LOSS AT ANODE | GAIN AT CATHODE | DEPOSIT IN STANDARD VOLTAMETER |
|---|---|---|---|---|
| *grams* | *amp.* | *mg.* | *mg.* | *mg.* |
| 8 | 0.500 | 2164.81 | 2164.78 | 2164.81 |
| 9 | 0.500 | 2092.12 | 2091.92 | 2091.83 |

The results show a surprising agreement. These were made using a pure crystalline deposit for the anode. With a striated deposit made from a filter paper contaminated solution the result was very different. The anode turned dark and had a very large amount of slime all of which was collected and added to the bowl. The results using a pure solution and this impure anode are as follows:

Anode lost.....2095.43 mg.
Cathode gained.....2092.21 mg.
Standard voltameter.....2091.83 mg.

The conclusion leaves no room for doubt that the purity of the anode is essential in such an experiment and this should be considered in voltameter work in general.

We have added to our complete paper a further discussion of the volume effect (see Part III) in reply to von Steinwehr's criticisms. (Zs. f. Instr., November, 1913) we have shown that this phenomenon of excess deposit in large bowls over small bowls, when impure electrolyte is used, is not confined to the porous cup form as von Steinwehr asserts, but has been observed in the filter form by Schuster and Crossley and in the siphon and Smith forms by ourselves. We have overcome this troublesome volume effect by *purifying the electrolyte*. We wish to lay great stress on the purity of the electrolyte used in obtaining the results

recorded in this paper. This question of the purity of the electrolyte has received less attention from previous observers than it deserves. We have described our methods for purifying silver nitrate in Part III of this series of papers.

GEOLOGY.—*The relative abundance of several metallic elements.* F. W. CLARKE and GEORGE STEIGER, U.S. Geological Survey.

During the past twenty-five years, several estimates of the relative abundance of the commoner chemical elements have been published from the laboratory of the United States Geological Survey.[1] These estimates, however, covered only such constituents of the earth's crust as are usually determined in the course of fairly complete analyses; including, in many cases, the less important elements barium, strontium, nickel, chromium, vanadium and zirconium. For the more familiar metals, copper, lead, zinc and arsenic, no really adequate data were available; and no attempt was made to compute either their abundance or their frequency. Such attempts have been made by others, however, but not altogether conclusively.[2]

In order to gain a definite idea as to the relative abundance of the elements in question, a number of *composite* analyses were made. That is, in each group of substances investigated, many samples were blended into one uniform sample, and that was then analyzed. The average content of each metal was determined in that way with as close an approximation to accuracy as could have been attained by many individual analyses. Four such composites have been studied thus far; namely, two of oceanic clays, contributed by Sir John Murray;[3] one of silt or mud from the delta of the Mississippi; and one of igneous rocks which had previously been analyzed in the laboratory of the Survey. For the the Mississippi silt the general analysis, not heretofore published, is as follows:[4] The composite was made up of 235 separate sam-

[1] For the latest of these estimates see Survey Bulletins 419 and 491. Also a paper in Proc. Amer. Phil. Soc., **51**, 214.

[2] See for example Vogt, Zeitsch. prakt. Geol., 1898, pp. 225, 314, 377, 413, and 1899, pp. 10, 274; and Kemp, Econ. Geol., **1**, 207.

[3] For the complete analyses of these clays see Journ. Geol., **15**, 783.

[4] Except when otherwise stated the analyses given here were made by Mr. Steiger.

ples, collected by E. W. Shaw. For the determination of NiO, CuO, PbO, ZnO and $As_2O_5$, 200 grams of the silt were taken. The presence of organic matter prevented the separate determination of ferrous iron. The chlorine in this analysis is doubtless due to salt from the Gulf of Mexico.

TABLE 1

COMPOSITE ANALYSIS OF 235 SAMPLES OF MISSISSIPPI SILT

| | | | |
|---|---|---|---|
| $SiO_2$ | 69.96 | Cl | 0.50 |
| $Al_2O_3$ | 10.52 | F | 0.07 |
| $Fe_2O_3$ | 3.47 | $Cr_2O_3$ | 0.01 |
| MgO | 1.41 | $V_2O_3$ | 0.02 |
| CaO | 2.17 | NiO | 0.017 |
| $Na_2O$ | 1.51 | MnO | 0.06 |
| $K_2O$ | 2.30 | BaO | 0.08 |
| $H_2O-$ | 3.78 | SrO | trace |
| $H_2O+$ | 1.96 | CuO | 0.0043 |
| $TiO_2$ | 0.54 | ZnO | 0.0010 |
| $ZrO_2$ | 0.05 | $As_2O_5$ | 0.0004 |
| $CO_2$ | 1.40 | PbO | 0.0002 |
| $P_2O_5$ | 0.18 | Organic | 0.66 |
| $SO_3$ | 0.03 | | 100.6229 |
| S | 0.07 | Less O | 0.13 |
| | | | 100.4929 |

For the four composite analyses above mentioned the data under immediate consideration are as follows:

A. The "red clay" of the oceanic depths. Composite of 51 samples, dredged from the sea bottom and representative of all the great oceans. The larger part of this material was collected by the *Challenger* Expedition. Determinations (by E. C. Sullivan) of CuO, ZnO, PbO and $As_2O_5$ made on 150 gram portions.

B. "Terrigenous clays," from oceanic depths of 140 to 2120 fathoms. Composite of 52 samples, namely 4 "green muds," and 48 "blue muds," also mainly from the *Challenger* Expedition. Determinations made on 300 gram portions.

C. Composite of 235 samples of Mississippi silt, as in the foregoing analysis. For the heavy metals 200 gram portions were taken.

D. Composite of 329 igneous rocks, all American. Determinations on 90 gram portions.

In the red clay a trace of molybdenum was also detected by Dr. Hillebrand.

TABLE 2

SUMMARY OF DATA FROM COMPOSITE ANALYSES

| | A | B | C | D | AVERAGE |
|---|---|---|---|---|---|
| NiO | 0.0320 | 0.0630 | 0.0170 | 0.00655 | 0.0296 |
| $As_2O_5$ | 0.0010 | trace | 0.0004 | 0.00074 | 0.0005 |
| PbO | 0.0073 | 0.0004 | 0.0002 | 0.00081 | 0.0022 |
| CuO | 0.0200 | 0.0160 | 0.0043 | 0.01167 | 0.0130 |
| ZnO | 0.0052 | 0.0070 | 0.0010 | 0.00638 | 0.0049 |

These figures give quite clearly the order of magnitude of the several percentages, and they are corroborated by the work of other investigators. In a series of 36 igneous and metamorphic rocks of British Guiana, Harrison found a mean percentage of 0.025 copper. In 23 of his samples lead was also sought for, and detected in 5 of them, the maximum amount being 0.02 per cent. In a typical specimen of the Columbia River basalt Wells found 0.034 of copper, and the same quantity was determined by Jensen in an andesite from Fiji. In the porphyries of Leadville, Colorado, Hillebrand was able to determine lead. Out of 18 samples, taken at points remote from ore bodies, the average amount found was 0.002 per cent of PbO. One porphyry also yielded 0.008 per cent of zinc oxide, and a rhyolite contained 0.0043 per cent.

In four rocks granite, porphyry and diabase from the Archean of Missouri, Robertson determined the following percentages of lead, zinc, and copper:

Pb.....0.00197 to 0.0068; average, 0.004
Zn.....0.00139 to 0.0176; average, 0.009
Cu.....0.00240 to 0.0104; average, 0.006

The adjacent limestones also carried these metals, but in slightly smaller proportions. Similar results were obtained by Finlayson from igneous rocks adjacent to lead mines in Great Britain. His averages are Pb, 0.0032 per cent, and Zn, 0.028. In the limestones and dolomites of the Dubuque region, Iowa, Weems determined lead and zinc. The average of nine samples gave

0.00326 per cent of Pb, and 0.00029 of Zn. Many other determinations of the heavy metals in rocks are scattered through the literature of geology but these examples are sufficient to illustrate what has long been known. The researches of Forchhammer, of Sandberger, and of Dieulafait are familiar to geologists, but they lack the quantitative basis which is supplied by the composite analyses given here.[5] The heavy metals are widely diffused throughout the crust of the earth, and generally in determinable proportions. The order of abundance, as now ascertained, appears to be Ni, Cu, Zn, Pb, As, with, of course, local variations.

With the aid of the estimate here given for zinc, which is near 0.005 per cent of ZnO or 0.004 Zn, it becomes possible to gain some notion as to the relative abundance of cadmium; for the two metals are commonly associated. In 10906 shipments of zinc ores from Webb City and Joplin, Missouri, mostly in carload lots, Waring[6] found an average percentage of 57.96 Zn and 0.358 Cd. The ratio is 1 Cd to 162 Zn. From 42 analyses of sphalerite given in Hintze's Handbuch der Mineralogie, the mean ratio is 1 to 163. From 82 analyses of European zinc ores, cited by Jensch,[7] the ratio 1 to 277 appears. The mean of these three estimates is 1 to 201; that is, in round numbers, zinc seems to be about 200 times as abundant as cadmium. A more precise estimate can hardly be made at present; but the figure is better than no estimate at all. It has a quantitative basis, and is therefore something more than a mere guess. If the percentage of zinc in the earth's crust is 0.004, then that of cadmium is of the order of 0.00002.

In the course of the regular rock analyses made in the laboratory of the Geological Survey, many determinations have been made of elements of minor quantitative importance. These determinations are numerous enough to fix their numerical significance between maximum and minimum limits as follows:

[5] For literature references see Survey Bulletin 491, The Data of Geochemistry, pp. 600–602, 643.

[6] Cited by Siebenthal in U. S. Geological Survey, Mineral Resources, 1908, 1, 796. See also, for other data, Waring's paper in Journ. Amer. Chem. Soc., **26**, 16.

[7] Ahren's Sammlung chem. techn. Vorträge, **3**, 201.

In round numbers, about 1200 such analyses, nominally complete, have been made and in 793 of them barium oxide was determined or proved to be absent. The mean of these determinations, counting absences as zero, is 0.0104 per cent, which is probably a maximum. If the remaining 407 rocks were all free from barium, and so regarded, the average percentage of BaO would be 0.069, a minimum; and between the two figures the most probable value would lie, their mean being 0.086. Upon this basis of computation the following table of percentages has been constructed.

TABLE 3

SUMMARY OF DATA FROM ROCK ANALYSES

| | NUMBER OF DETERMINATIONS | MAXIMUM | MINIMUM | MEAN |
|---|---|---|---|---|
| BaO | 793 | 0.104 | 0.069 | 0.086 |
| SrO | 649 | 0.040 | 0.022 | 0.031 |
| $Li_2O$ | 581 | 0.011 | 0.005 | 0.008 |
| NiO | 299 | 0.026 | 0.006 | 0.016 |
| $Cr_2O_3$ | 293 | 0.050 | 0.012 | 0.031 |
| $V_2O_3$ | 102 | 0.026 | 0.002 | 0.014 |
| $ZrO_2$ | 372 | 0.023 | 0.007 | 0.015 |

In three of the composite analyses already cited, similar determinations were made, and the results obtained fit in fairly well with the figures of this table.

The percentages found are shown in the following table:

TABLE 4

DATA FROM ANALYSES OF COMPOSITE SAMPLES

| | RED CLAY | TERRIGENOUS CLAY | RIVER SILT |
|---|---|---|---|
| BaO | 0.17 | 0.05 | 0.08 |
| SrO | 0.046 | 0.025 | trace |
| NiO | 0.032 | 0.065 | 0.017 |
| $Cr_2O_3$ | 0.01 | 0.044 | 0.01 |
| $V_2O_3$ | 0.028 | 0.028 | 0.02 |
| $ZrO_2$ | undet. | undet. | 0.05 |

The data so far obtained may not be final; but they clearly indicate the several orders of magnitude which it was sought to determine.

## ABSTRACTS

**Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. Each of the scientific bureaus in Washington has a representative authorized to forward such material to this journal and abstracts of official publications should be transmitted through the representative of the bureau in which they originate. The abstracts should conform in length and general style to those appearing in this issue.**

BOTANY.—*Annona sericea and its allies.* WILLIAM E. SAFFORD. Contributions from the United States National Herbarium, **16**, Part 10. pp. 263–275. December 13, 1913.

In continuance of his studies in the Annonaceae, the writer finds that the silky annona of French Guiana (*Annona sericea* Dunal) is the type of a natural subgroup of the genus Annona, that should be segregated as a section, for which he proposes the name Pilannona to give it co-ordinate rank with Euannona, Atta, Ilama, Annonella, and Chelinocarpus. As in other natural plant groups there are certain species which appear to form connections with allied groups, so in the section Pilannona the type species, *Annona sericea* Dunal, which has normally 3 petals, appears to be allied to the 6-petaled *A. paludosa* Aubl., while at the opposite end of the series *Annona jamaicensis* Sprague approaches *A. cherimola*, belonging to type section Atta. In addition to descriptions of the principal species included in this section photographs of a number of original type specimens are presented, including that of *Annona echinata* Dunal in the De Candolle Herbarium at Geneva, and the flower of *A. sericea* described and figured by Dunal in his classical monograph of the Annonaceae. Among the new species described and figured are *Annona jenmanni*, from British Guiana; *A. trinitensis*, from the island of Trinidad; *A. longipes* from southern Veracruz, Mexico; *A. holosericea*, from the Pacific coast of Costa Rica; *spraguei*, from Panama; *Annona cercocarpa*, from Colombia and *acuminata*, collected on the Isthmus of Panama in 1861 by Dr. Sutton Hayes. In addition to the above *Annona jamaicensis* Sprague is redescribed and illustrated with two photographs.

This paper deals with the taxonomy of the species described. It will be followed shortly by a more comprehensive one on the "Classification

of the genus Annona with descriptions of new and imperfectly known species," which will form part 1 of the Contributions from the United States National Herbarium volume 18. Both papers are preliminary to an account of the economic custard-apples and their allies, to be published by the United States Department of Agriculture.

W. E. S.

BOTANY.—*Descriptions of new plants, preliminary to a report upon the flora of New Mexico.* E. O. WOOTON and PAUL C. STANDLEY. Contributions from the U. S. National Herbarium **16,** 109–196. pl. 48–50. 1913.

For some time the authors of this paper have been engaged in the preparation of a Flora of New Mexico. This work, which has now been completed, will, it is expected, be published in the near future. The only general descriptive manual covering any portion of the arid southwestern United States heretofore available has been Coulter's Botany of Western Texas (Contr. U. S. Nat. Herb., vol. 2), a work which unfortunately is now out of print.

Little attention having been given by taxonomists to the flora of New Mexcio in recent years, it was to be expected that many undescribed species would be discovered during the progress of the investigation just mentioned. These are described in the present paper, partly to reduce the bulk of the complete Flora and partly to prevent confusion on the part of those who wish to use it. Altogether 198 species are described as new. The largest number in a single family (50) is found in the Carduaceae, the group which has the largest number of representatives in the state. In this family a new genus, Herrickia, is named, based upon a plant of northern New Mexico, related to Aster and Xylorrhiza. Among the other plants described may be mentioned two new Yuccas, an Agave from the Organ Mountains, two oaks, six species of Padus, a Robinia (the second species of this genus known from the western United States), eight species of Gaura, five of Penstemon, eight of Erigeron, and four thistles. In addition to the new species described, 65 new names or new "combinations" are published.

P. C. S.

# PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

## WASHINGTON ACADEMY OF SCIENCES

The 16th annual meeting of the Washington Academy of Sciences was held at the Cosmos Club, January 15, 1914, with President Tittmann in the chair. The following were elected officers for the ensuing year: *President*, DAVID WHITE; *Corresponding Secretary*, GEORGE K. BURGESS; *Recording Secretary*, W. J. HUMPHREYS; *Treasurer*, E. W. PARKER; *Vice-Presidents*, Representing the Anthropological Society: F. W. HODGE; Botanical Society: F. L. LEWTON; Entomological Society: A. L. QUAINTANCE; Electrical Engineers Society: E. B. ROSA; Foresters Society: W. B. GREELEY; Historical Society: J. D. MORGAN; Biological Society: PAUL BARTSCH; Chemical Society: M. X. SULLIVAN; Engineers Society: G. W. LITTLEHALES; Philadelphia Society: L. A. FISCHER; Geological Society: F. L. RANSOME; Archaeological Society: MITCHELL CARROLL; *Non-Resident Vice-Presidents*, IRA REMSEN and J. M. COULTER; *Managers, Class of 1917*, A. H. BROOKS and L. O. HOWARD.

The report of the Corresponding Secretary showed the total membership as 369, an increase of 14 during the past year, and that the Academy has lost by death the following: Dr. J. R. Eastman, the first President of the Academy; Prof. Alexander Macfarlane; Dr. Robert Fletcher; Gen. C. W. Raymond.

The Treasurer's report showed the total receipts $4805.69, disbursements $3657.82, cash on hand $1147.87, investments $12,090.

The report of the Auditing Committee was also received.

The following were elected to resident membership: Erwin F. Smith, L. H. Dewey, W. W. Stockberger, Haven Metcalf, Bert S. Butler, Monroe Hopkins, W. R. Blair, C. W. Burrows, W. Blum, J. T. Kelley, Jr., Paul J. Fox, Dr. W. W. Randall, J. F. Mitchell, W. Salant, A. Seidell, C. S. Hudson, W. N. Berg, G. E. Patrick, R. O. E. Davis, Percy H. Walker, W. W. Skinner, W. H. Waggaman, H. C. Gore, Rene de M. Taveau.

President-elect White then took the chair and President Tittmann delivered an address on "Our Northern Boundaries" (this Journal **4**, 37, 1914).

GEORGE K. BURGESS, *Corresponding Secretary.*

## THE BIOLOGICAL SOCIETY OF WASHINGTON

The 516th meeting was held November 15, 1913. Vice-President Paul Bartsch in the chair and thirty-five persons present.

F. V. COVILLE presented a communication on *The physiology of the blueberry*. His remarks were based on wide experience in green house and outdoor culture of this plant. Three conditions are essential to its successful propagation: (1) an acid soil; (2) the presence of the micorrhizal fungus to enable the plant to obtain nitrogen; and (3) the stimulating effect of cold on the twigs while they are dormant. The last is a condition of vital importance, associated as it is with the transformation of starch into sugar. As a result of this series of experiments, the commercial propagation of the blueberry is now possible. Very large berries have been developed, some of them from $\frac{1}{2}$ inch to $\frac{7}{8}$ inch in diameter. The various means of cultivation were explained and illustrated by means of numerous lantern slides.

W. C. KENDALL, the second speaker announced on the program, was absent, and the chairman asked DR. LEON J. COLE of the University of Wisconsin to address the Society. He responded by giving an account of his experiments in breeding pigeons for the study of color inheritance.

Owing to the lateness of the hour, the communication by BARTON W. EVERMANN was postponed.

The 517th meeting was held November 29, 1913. President E. W. Nelson in the chair and sixty-three persons present.

The meeting was devoted to a discussion of Parallel Development. A. D. HOPKINS read a paper on *Parallelism in morphological characters and physiological characteristics in Scolytoid Beetles*. He had made a special study of these beetles and his ideas of parallelism in nature were largely founded on evidence they have furnished. He defined the subject as follows:

"Parallelism in morphological characters and physiological characteristics in Scolytoid beetles relates to the occurrence of the same or similar elements of structure or the same kind of activity in two or more species, genera, subfamilies, or families. Parallel species, genera, and larger groups are those in which structure or habit is in many respects alike. Such species or groups may be closely allied or more or less widely separated. Universal parallelism relates to repeated or multiple origin, development, and evolution of the same or similar inorganic or organic forms or activity.

"This tendency towards parallel development appears to be in accordance with a fundamental principal or law of *parallelism in evolution*, under which the origin and evolution of the same form or activity, under the same or similar physical influences, has been repeated many times; or, in other words, that under similar environments, needs, and requirements in nature, independent development and evolution from

a common base may produce repeatedly the same or similar morphological and physiological results."

Numerous examples were given and illustrated on the board, of characters of structures and characteristics of habit which were paralleled over and over again in connected and disconnected genera, subfamilies, and families. He also illustrated characters and characteristics which were paralleled in all of the species of a single genus and in connected genera, groups, subfamilies, and families, and said further:

"Thus we see that parallel modification in morphological and physiological elements is an important factor to be considered in taxonomy. It is evident from a comparative study of the various systems of classification that the failure of taxonomists to fully realize its importance has led to many erroneous conclusions and much confusion.

"In conclusion, it seems to me that we have two fundamental questions to be answered in regard to the origin, evolution, and classification of organisms.

"1. Are the taxonomic characters and characteristics of the species, genus, family, order, class, and kingdom *the result of phylogenetic descent from a single ancestral nucleus, thru natural selection and the inheritance of selected characters?* or

"2. Are they *the result of phylogenetic descent from many nuclei thru natural selection and natural parallelism?*

"I am inclined to the belief that an affirmative answer to the second question would be more nearly in accord with natural law."

President NELSON exhibited a series of mammal skins in pairs similar in outward appearance but widely different in structure and classification, showing the parallelism of shape and color.

O. C. OBERHOLSER discussed parallel development as illustrated in birds. He showed a large series of specimens in which resemblances in form and color were very striking.

J. W. GIDLEY pointed out the difference between parallel and convergent development.

Messrs. LYON, HAY, BAKER, BARTSCH and GILL took further part in the discussion.

Dr. HOPKINS closed the discussion by stating that the illustrative material presented by Messrs. NELSON and OBERHOLSER had shown only one phase of the principle—homomorphic parallelism, which is not correlated with evidences of natural affinity, but is repeated in more or less widely separated groups and species, while the other—homologous parallelism—is parallelism of structure, color, habit, etc., which is correlated with evidence of natural affinity and is repeated in the same species or in connected genera or larger groups.

The 34th annual meeting and 518th regular meeting was held December 13, 1913, President E. W. Nelson in the chair and twenty-eight persons present.

The annual reports of officers were received. The election of officers

for 1914 took place and resulted as follows: *President*, PAUL BARTSCH; *Vice-Presidents*, J. N. ROSE, A. D. HOPKINS, W. P. HAY, MARY J. RATHBUN; *Recording secretary*, D. E. LANTZ; *Corresponding secretary*, W. L. MCATEE; *Treasurer*, WELLS W. COOKE; *Members of council*, WILLIAM PALMER, HUGH M. SMITH, VERNON BAILEY, MARCUS W. LYON, JR., N. HOLLISTER.

The president, Paul Bartsch was selected to represent the Society as a vice-president of the Washington Academy of Sciences.

The president appointed as Committees on Publication the following: N. Hollister, W. L. McAtee, Wells W. Cooke.

D. E. LANTZ, *Recording Secretary.*

XU